

the mucous membrane of the cardiac extremity of the stomach, and sometimes in that of the pyloric extremity.

Simple obstruction to the current of blood during the agonies of death, could never produce such appearances, for if it could, we ought to meet with them invariably. On the contrary, where an obstruction does occur, which I am disposed to believe is very uncommon in natural death, whether this obstruction arises from the liver or from the lungs, the arrest of blood should be manifested by an uniform tinge of red in the organs concerned, and not by patches here and there. Such marks of obstruction should also be more frequently seen and better developed, in cases of enlarged, indurated liver, in consumption, in dropsy of the thorax, and in all cases of obstruction of the lungs.

In concluding these observations, I should do injustice to an artist of uncommon merit and ingenuity, were I to withhold the marked expression of my sense of the invaluable services rendered by Mr. Drayton, in promptly and faithfully executing the drawings of the parts represented in the plate, and in subsequently executing the engravings; I have also to acknowledge the characteristic liberality of the publishers, in freely allowing me the advantage of an unrestrained use of his talents.

ART. II. *On the Functions of the Capsulæ Renales*. By JOHN REDMAN COXE, M. D. Professor of Materia Medica and Pharmacy in the University of Pennsylvania.

IT is now nearly three centuries\* since EUSTACHIUS published his book "*De renibus*," in which, for the first time, the *capsulæ renales* are pointed out and described. Notwithstanding, however, the long period that has elapsed since their discovery, their use in the animal œconomy still remains a mystery, almost as many offices having been attributed to them, as there have been writers on the subject. It is my intention to point out what I conceive to be their function; and, although, when I first took up its consideration, I believed the opinion I entertain was original; yet, on recurring to our authorities in anatomy, I found that others had conceived a similar idea, though the opinion seems not to have been very zealously maintained, even by the individual who first suggested it; while it had been most powerfully opposed by every other writer. MOLINETTI, who flourished

\* In 1563.

about a century and a half since, and CHARLES BELL, are the only anatomists who seem to have advocated the opinion, that the *capsulæ renales*, were merely diverticula of blood, from the kidneys, to prevent the secretion of urine in the fœtal state; yet so little have they said to substantiate their opinion, that it appears more like a random thought, than the result of a deliberate and serious reflection.

It being obvious that both in a physiological and pathological point of view, a correct opinion of the functions of any part of the body, is desirable, and must be of practical utility to the physician and surgeon, on many occasions; any attempt to fix the functional character of these bodies, must be considered as advantageous.

The *capsulæ renales* are almost universal in quadrupeds, whether carnivorous or herbivorous—in birds—in cold-blooded reptiles; and perhaps, we may say, generally, in every animal, in whom a renal conformation exists. By consulting HALLER's *Physiology*, 7. p. 286, et sequent. we find that different anatomists have noticed them, in the dog, fox, civet, lion, tyger, bat—the horse, ox, elephant, sheep, goat, stag, hedge-hog, porcupine, mole, dormouse, sow, marmot, and many others—in the serpent, viper, tortoise, &c. and in the eagle, vulture, crow, duck, swallow, &c. Now from this we infer, that what is so universal, must be of some great and general utility; and that any explanation of the functions of these bodies, must indubitably rest, in a great degree on this fact. In man, they have never, that I can learn, been found wanting; nevertheless, anomalies in this respect, may have occurred, although unnoticed. They rarely differ in number, being usually the same as the kidneys themselves. It may, however, be an interesting subject of investigation, whether, in case of a single kidney, more than one capsule exists; or if three or more kidneys are found, an equal number of these capsules are not also present. BARTHOLINE mentions a case in which four were seen by him; though Haller thinks it probable that two of them were only lumbar glands. He tells us also, that MORGAGNI saw three; and RHODIUS, two on the left side. These anomalies are, however, of little comparative importance; and we may venture to affirm, that, except in a few cases of monstrosity, the number of these bodies will always be found equal to that of the kidneys; and this fact serves in some degree to establish their intimate connexion with, or subserviency to those glands, in some period of existence.

I shall enter into the anatomical structure of these organs, so far only as is requisite to elucidate the position I assume, as to their use in the animal economy; referring those who feel an interest in the inquiry to our systems of anatomy for further particulars.

From the general description of all anatomists, we learn that on the upper edge of each kidney, is found a particular body, having some analogy to a gland, but differing therefrom in *no excretory duct having ever been detected*. Its form is very various, and probably dependant on the age of the subject, it having been seen quadrangular, triangular, orbicular, and oval. It is somewhat like the kidney, being larger above, and ending, as some have described it, in a kind of conic figure; all which varieties, as above remarked, are probably owing to the age of the individual in whom they have been examined. What is of greater importance, is, that with very few exceptions, they are always larger in the foetal than in the adult state; in fact, before birth, they are mostly larger than the kidney itself, but gradually diminish in size, after birth; and in the adult and aged subject, they are very small, when compared with the kidneys themselves.

These organs are uniformly described as being largely supplied with blood-vessels; the arteries, coming from the aorta, and the renal and diaphragmatic arteries; the veins from the vena cava and from the renal and diaphragmatic. Some slight variation is occasionally found; for the vessels are not always alike numerous, nor do they invariably proceed from the same sources.

The interior of these bodies has a narrow, triangular cavity, varying in size, probably from the difference of age; and in colour likewise from the same cause. A granulated or follicular substance usually fills the cavity, which cavity is connected with the veins, as it may be distended by blowing air into those vessels. The capsular vein communicates with the inside of the capsule, like the splenic vein with the cells of the spleen.

In conformation, these bodies are generally more regular, solid, and consistent in the foetus and young children, than in adults. In old age, they often become flaccid; and are much decayed.

From these data it would seem correct to infer, that, whatever be their use, that use is restricted chiefly to the foetal state, and that all inquiries as to their physiology must chiefly be limited to that period of life.

Eustachius, their first discoverer, called them *glandulæ renibus incumbentes*. Bartholine gave them the name of *capsulæ atrabiliaræ*, CASSERIUS that of the *renes succenturiati*, and DIEMERBROECK, *glandulæ renales*: but all these names, founded on their presumed uses, are manifestly incorrect. The non-existence of an excretory duct, proves them not to be glands within the genuine acceptation of the term; and the uses assigned by different writers to them, not having been proved, the names assumed cannot be considered as well-founded, and ought to be expunged from the systems of anatomy.\*

\* See note at the end of this essay.

Any person desirous of informing himself fully on the subject of these bodies will find an ample detail in the *Dict. des Sciences Médicales*, v. 47, p. 412, in which, all that has heretofore been said of them is reported. We learn, that, as before stated, they are much developed in the foetus; but that, as soon as birth takes place, the descent of the diaphragm in respiration impedes their further growth, and from that time they may be said to diminish in size, whilst that of the kidneys progressively augments. There can, I think, be little doubt that the pressure of the diaphragm upon them, must have a considerable influence in diminishing the flow of blood to them; and thereby compel that fluid to pass in a greater amount to the kidneys, when their functions as diverticula are thus arrested. This serves in a measure, to explain why the use they may have exercised in the foetal state is no longer required after birth—the blood now passes directly to the kidneys, in larger amount; and vessels, heretofore merely kept pervious, are enlarged, by the cessation of the office of the capsules now no longer required. In this we perceive some analogy with the lungs, through which, undoubtedly, a small portion of blood must flow, to keep those vessels pervious, which, after birth, are destined to circulate the whole sanguineous stream, when the diverticula of the ductus arteriosus and foramen ovale are suspended in their operation.

Molinetti, as I have already stated, considered the renal capsules of use, only to *prevent the secretion of urine* in the foetus; his luminous conception of the subject seems, however, to have met but little attention; objections, indeed, were raised against his opinion, which appear to me more specious than valid. Thus it has been asserted that the renal capsules ought not to be looked upon as simple receptacles—as there would have been no occasion for their wonderful structure on this account. Certainly this objection, backed by no other proof, than what the author thinks ought to be the case, can scarcely be deemed conclusive. The spleen, in all probability, with a formation equally wonderful, serves some similar purpose in the animal economy.—It has been also affirmed, that it is *certain*, (of which no proof is given,) some secretion is performed in them. Now, if this be the case, it is most extraordinary that no excretory duct has ever been discovered in all the attempts for this purpose; and HALLER doubts whether they do secrete a humour peculiar to themselves. No fluid is found in them in the adult state; although in children, and especially in the foetus, they do contain a small quantity of a yellowish or reddish humour, the remains probably of the blood that passed there. Another objection to Molinetti's opinion, is, that it supposes the foetus to make no

water, which it is presumed is overturned by the affirmed discovery of the allantois, by BIDLOO. Now I believe this idea, at least as it respects the human foetus, is no longer maintained; and as it relates to the analysis of the amniotic fluid, we find little to countenance the presence of urine or its constituents, therein, which might be presumed to be mixed with it, through the medium of the urethra. It has also been said, that it could not be conceived, how such small arteries should intercept so great a quantity of blood. To this it may be said, that the arteries, if small, are numerous, and the foetal pulse is probably infinitely more rapid than after birth. At any rate, it is an objection not established by any fact, and may therefore well be considered as of no force.

Let us, however, consider for a moment a question of fact, viz.: Does the foetus in utero secrete urine?

Bartholine, speaking of the membranes of the foetus, adverts to the allantois, (a term derived from the Greek *αλλας*, signifying a sausage, from its being a sort of elongated vessel, which does not surround the whole foetus, but is rather like a girdle,) situated between the chorion and amnion, and communicating with the bladder by means of the urachus. Its use, according to Bartholine, is to receive the urine in brutes; for in man, he tells us, it is not present. In man, he adds, the amnion receives the urine, mixed with the sweat. This membrane has however been denied existence even in brutes, by some writers; its existence is nevertheless said to be very manifest in brutes during gestation, (*Dict. des Sc. Med.* 1. p. 410,) but is with difficulty seen in the human ovum, being only met with from the second to the fourth month of gestation, nor are we sure that it communicates with the bladder. We may moreover add, that some writers have endeavoured to prove, that, even in animals, it is not intended to receive the urine, but that it transmits to the bladder, by the urachus, the fluid it contains, to serve for nourishment to the young individual. We may I think safely set this down as hypothetical, and admit that the particular use of the allantois is altogether unknown. If then the human foetus wants this receptacle, (which indeed for the mere purpose of receiving the urine, seems altogether unnecessary, in either man or brutes, as the bladder might be presumed adequate,) it should follow, that any urine secreted in the foetal state, ought to be found either in the bladder, or intermixed with the liquor amnii. We have however no reason to believe that this last is the fact, since the analysis of that fluid by BUNIVA, VAUQUELIN, BERZELIUS, and others, proves sufficiently that all the characters of urine are wanting in it, (see Johnson's *Animal Chemistry*;) and as it respects the existence of urine in

the bladder, no fluid of any amount has been detected therein. What little has been found, seems indeed to want the requisite of urea and of many of the saline ingredients which are noticed in it after birth. Independently of this too, the amount is far too small for any reasonable views on the subject, founded on calculation. Haller states the quantity of urine discharged by the adult, to be from twenty-eight to sixty-four ounces in the twenty-four hours. Now, if we consider the foetus as only beginning to secrete it when five months old, it will leave four months or one hundred and twenty days to the period of birth. Suppose we take the moderate quantity of only half an ounce per day, secreted during that space of time. This will be nearly equal to four pounds, which ought to be found in the bladder or in the liquor amnii at birth; but we have stated that analysis gives no evidence of the presence of urine in the last; and instances are recorded in which the urethra was closed at birth, but in which no fluid was present in the bladder. We have, therefore, a right to ask of those who contend for this secretion in the foetal state, conclusive evidence of the fact, but which we believe cannot be given, nor any satisfactory channel pointed out by which it may have escaped. We may then admit, I think, that in the period of gestation, only so much blood percolates the vessels of the kidney, as will suffice to keep them pervious for their important functions after birth; whilst the full amount which, according to the laws of circulation, would otherwise be conveyed through them, is diverted off, by the renal capsules, and carried immediately back into the vena cava.

VAUGHAN in his anatomy, 2, p. 132, adverting to this opinion, that the capsules are to be regarded as diverticula in the foetus, to divert the blood from the kidneys, and lessen the amount of urine, adds, "but if so, why do they remain in adults, and not disappear like other diverticula?" Now what are the diverticula of whose functions as such we are assured? The ductus arteriosus and foramen ovale, are perhaps the only unquestionable ones we are acquainted with, and their vestiges are conspicuous to the latest period. The thymus gland is probably of a like nature, and it never entirely disappears. If the spleen is of a similar character, we find it continuing its functions to the end of life. Is there not however full as much necessity apparently, for a diverticulum for the blood from the kidneys in the foetal state as from the lungs? Both of these organs seem to come fully into operation only after birth; and of all the uses ascribed to the capsules, not one is found to quadrate with fact, as well as with reason, but that which ascribes to them the functions of diverticula, as a wise provision of nature, to guard the



kidneys from the overflow of blood, which, by the common laws of circulation would otherwise be carried to them. All other uses are incongruous, and entirely hypothetical, with scarcely a solitary fact to give them the slightest credit. As to their continuance in the adult state, notwithstanding their evident and acknowledged decrease, it is very possible, nay probable, that their functions are never absolutely terminated. The numerous instances recorded, of immense amount of urine in the bladder in certain cases of retention, and the surprising instances of long-continued suppression of urine, appear, alike to prove that some diverticulum has been called into operation to arrest the fatal issue of those complaints. If, in cases of retention, the average amount of two pounds may be supposed to be secreted daily, as formerly adverted to, then, in proportion to the number of days during which the disease continues, we ought to find in the bladder an equal number of quarts of that fluid; but is it ever in that amount? Does not nature, here, in order to obviate in a degree the effects of disease, enable the renal capsules to resume their original functions? I think it at least probable. Again, in cases of suppression for days, what becomes of the blood determined to the kidneys, if not conveyed away by some other channel? How could a total suspension of the secretion of urine ensue, the kidneys being found after death in many instances free of disease? Death indeed does not always follow even the most extended cases of this nature, of which the following are a few of many instances on record. *La Motte*, (*Traité complet des Accouch.* p. 567,) mentions the complete suppression in a woman of sixty years, for seventeen days. The first discharges on recovery being bloody, but gradually passing to the state of urine. *Desault*, (*Traité des Mal. des voies Urin.* p. 32,) says, that in hysteric affections, the suppression has continued for more than forty days. In the *Dict. des Sci. Med.* 47, p. 429, are several cases—amongst others, one of a patient of fifty years, reported by *M. de CLaubry*, continuing for nine days, and another from *Dr. VIEUSSEN*, of a little girl, eleven years of age, who had for eighteen months a total suppression, from which she recovered. In both cases, the suppression of so important a secretion, was unaccompanied by those frightful symptoms commonly existing. There is another still more extraordinary case given, of a suppression continuing for seven years, from which the patient recovered, and died many years afterwards of another disease. Now, in all these cases, it is scarcely possible to resist the impression, that the danger was obviated by means of a diverticulum—and if so, what more probable than the resumption of their original functions by the renal capsules.

If more cases of a like character are required, they may be found in the philosophical transactions of Great Britain—especially one of a boy, who never made water, but lived in perfect health to his seventeenth year, a constant diarrhœa attending. This is related by Dr. Richardson, V. 28, p. 167.

BOERHAAVE had an idea of the urine that it served as a means of expelling the portion of our humours tending to putrefaction, and which is calculated to bring others into the same septic disposition—how far this is correct, I shall not pretend to say; our present views in relation to it, seem to be, that it is intended to convey away the salts introduced into the system, or which are formed by the actions of life. If either view be adopted, we may perceive, why, in the foetal state, the secretion of urine can scarcely be said to take place, inasmuch as any, then formed, is deficient in the characteristic property of urea, a substance of a very singular nature, and consisting of a predominancy of nitrogen, which is nearly 40 per cent. in amount of that principle. The kidneys may then be regarded as the principal channel for conveying off the nitrogen, as the lungs for carbon, and perhaps the liver for hydrogen; but as this principle does not predominate in the foetal state, it of course does not require to be then discharged, and the kidneys only take on their perfect functions, when an excess of nitrogen is introduced, requiring elimination, viz. after birth. Of course, all that is requisite in the foetal state, is merely that the vessels of the kidneys be kept pervious, whilst the excess of blood, carried off by the diverticulum of the renal capsules, finds its way to the mother, and has its azotic portion eliminated through her kidneys.

I shall only add further, that these organs are the seat of disease, as demonstrated by dissection; thus GREISELIUS found the left capsule, in a man of forty-five years old, of great size; concealing an abscess filled with twelve pounds of putrid sanious matter, which being evacuated, the capsule weighed more than two pounds. PORTAL mentions a boy of two months, in whom the capsule was filled with a liquor like the blackest ink, and was larger than a pigeon's egg. BLASIVS mentions a woman of fifty years old, in whom the capsule was equal to the fist, soft, reddish, and filled with black purulent matter, and with much gravel and sand; and Bartholine states the right capsule in a melancholic patient, to have been longer and larger than the spleen, and to have had a double cavity.

Upon the whole, after duly considering the subject, I am more than ever convinced, that no other use can be ascribed to the renal capsules, than that of diverticula in the foetal state; and that the pro-



bability is, that their functions are never entirely suspended; at least, that on many occasions of disease, they resume in a certain degree their former functions, and thereby co-operate in suspending the fatal issue of many cases, which would otherwise occur. If I am correct in the positions thus advanced, I think all the names by which these organs have been hitherto distinguished, should be abolished, and that of *diverticula urinæ* should be substituted in their place.

## NOTE.

Perhaps a succinct account of the various opinions of the uses of these organs, may not be improperly introduced here; that the reader may compare the probabilities of each. *Eustachius*, their discoverer, seems to have acted with uncommon modesty, in not venturing to assign any use to them, that I can find; he merely calls them the renal glands, from their vicinity to the kidneys. *Casseri*, *Bauhin*, and others, were persuaded they aided in the secretion of urine—hence the name of *succenturiatæ glandulæ*. *Spigelius* supposed them made to fill up the space between the kidneys and diaphragm, to support the stomach in the part above the emulgent veins and arteries; and also to absorb the moisture that sweats from the neighbouring vessels. *Wharton* and *Riolan*, by whom they were named *glandulæ ad plexum positæ*, gave them that appellation, because they cover the semi-lunar ganglions, and the beginning of the plexus, which the great intercostal nerve forms in the belly; and they thought their sole use was to support these plexuses—considering, that otherwise they would have pressed too much on the emulgent veins. *Sylvius* imagined an acrid juice was separated in the capsules, which, mixed with the blood returning from the kidneys, after the secretion of urine, served to dilute it and excite the action of the vena cava upon it. The elder *Bartholine*, (*Gaspard*,) having found them filled with a fluid of an obscure brown colour, believed them to be the secretory organ of the atrabilis; and that this humour was taken up by the capsular veins, and carried to the kidneys by means of the renal veins; hence he gave them the name of *capsulæ atrabilaris*. *Gaspard* was unacquainted with the circulation, and is considered excusable in his ideas; but *Thomas*, his son, in defending the opinion, in opposition to his knowledge of the circulation, says, that the atrabilis, thicker and heavier than the blood, might have in the capsules and renal veins, a *retrograde motion* from that of the blood, in order to reach the kidneys—and *Petrucchi*, a Roman physician, not much more than half a century past, even affirmed that he had discovered valves in the capsular veins, disposed contrary to those of other veins, and fitted to favour the passage of the atrabilis to the kidneys. *Bartholine* moreover considered them as separating from the blood, the colouring matter of urine.

Some authors have made them the seat of various passions. *Collins* says their use is to impart a fermentative liquor flowing out of the termination of the nerves, by some direct passages into the body of the glands belonging to the kidneys, to dispose the blood in order to the secretion of the serous and saline parts from the vital liquor. *Kerkringius* ascribed to them a power of secreting a juice fitted to colour and animate the blood. *Valsalva* thought he

had discovered their communication with the testicles, and that they aided in the seminal secretion. *Senac* supposed they secreted the fetal meconium—and *Van Helmont*, that they secreted a juice endowed with lithontriptic powers, created by his archæus, to prevent the formation of renal calculi. *Lieutaud* viewed them as separating an acrid, penetrating liquor, very fit to prevent the formations of concretions in the vena cava. *Boerhaave* assigned them the function of correcting in the blood that flows from the kidneys, the fluidity of that liquid, impoverished by the loss which the secretion of urine causes. *Thury* thinks they serve for almost the same use with the *membrana adiposa*; straining out a liquor, which by its long stay, becomes oily, then throws it into the veins, whereby the blood, stripped of its fluidity by the secretion of its serous parts recovers its former state, &c. so that they do the same office to the blood of the cava, that the omentum does to that of the porta.—These are probably sufficient, I shall therefore only further mention *Morgagni's* opinion, or rather conjecture, for as such does he offer it, viz. that probably a humour filtered into the cavities, fitted to fill the receptaculum chyli and thoracic duct whilst the infant remained in utero, a time in which these parts receive no chyle from the intestines.

The opinion of *Molinetti*, which I most cheerfully credit him with, is in my estimation, so very superior to all the above crude and indigested notions, that I shall say no more on the subject. *Morgagni's* is the only one, I should regard in the least plausible; and I think, had he ascribed to the, (so called,) thymus gland, those functions thus given to the renal capsules, that he would have probably been correct in such an opinion; an opinion I believe not before taken of that organ, but which a frequent reflection on the subject has led me occasionally to adopt, although I am far from laying any stress upon it, and consider it, (as *Morgagni* did his views of the uses of the capsules,) more in the light of conjecture, than as being founded on any fact of sufficient importance to give it the character of truth, further, than the apparent necessity of some fluid passing through the thoracic duct and receptaculum chyli, in order to keep them pervious for their important office after birth. By an accidental reference to *Horner's Anatomy*, I think the position assumed of the functions of the capsules is considerably strengthened. In a note, the doctor mentions the presence of this body, in a case of deficiency of the kidney.

ART. III. *Case of Paruria Erratica, or Uroplania*.\* By SALMON A. ARNOLD, M. D. of Providence, R. I.

MARIA BRENTON, aged 27 years, of sound constitution, generally enjoyed good health until June, 1820, when she was afflicted with a suppression of the catamenia, accompanied with hæmoptysis. The medical attendants, irregular practitioners, bled her profusely

\* Some account of this very singular case, has been published in a Journal of limited circulation.